

OBITUARY NOTICES
OF
FELLOWS DECEASED.

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SIR GEORGE HOWARD DARWIN, 1845-1912.

By the death of Sir GEORGE HOWARD DARWIN, which took place on December 7 last, the Society has lost an investigator of rare skill and untiring patience, whose work has done much to add lustre to a name already pre-eminent in the annals of British science.

Sir George, the second son of Charles Darwin, was born at Down, Kent, in the year 1845. Brought up amidst scientific surroundings from the start, he received his early education privately at the hands of Rev. Charles Pritchard, who afterwards became Savilian Professor of Astronomy in the University of Oxford. Among Pritchard's pupils at the time were numbered the sons of many of England's leading scientists, and many of these in turn have since won for themselves distinguished careers, no fewer than three having officiated in after years as Presidents of the British Association.

He gained an entrance scholarship and entered at Trinity College, Cambridge, in 1864, and graduated as Second Wrangler and Smith's Prizeman in the year 1868. The same year he was elected to a Fellowship at Trinity College. The Senior Wrangler of the year was Mr. Fletcher Moulton of St. John's (now Lord Justice of Appeal), who relates how he himself at first remained at Cambridge with the object of pursuing an academic career while Darwin proceeded to London to read with a prominent barrister with a view to adopting the bar as a profession. He was duly called but never practised, and a few years only elapsed before the positions were interchanged and Darwin once more returned to Cambridge, where the rest of his life was spent. There he devoted himself to the solution of those intricate problems, associated primarily with the unravelling of the early history of the Solar System, which form the subject of the four monumental volumes of 'Collected Papers' recently issued under his personal supervision by the Cambridge University Press.

This work had already made considerable progress when he was invited to occupy the Plumian Chair of Astronomy, which became vacant by the death of Challis in 1883, and in the following year he was re-elected to a professorship at Trinity, his previous tenure having expired by lapse of time in 1878. The same year he married Maud, daughter of Charles du Puy, of Philadelphia, and took up his abode at the pleasant home of Newnham Grange at the end of the "Backs," which will long be associated with kindly recollections by all whose privilege it was to visit there. He had two sons and two daughters. His elder son Charles followed in his father's footsteps by becoming a scholar of Trinity in 1905 and graduating as Fourth Wrangler in the Mathematical Tripos of 1909.

Darwin's earliest notable contribution to science was an investigation "On the Influence of Geological Changes on the Earth's Axis of Rotation" ('Phil.

Trans., 1877, Part I, vol. 167). At this time the opinion was frequently held by geologists that the wanderings of the pole in the Earth's figure caused by geological upheavals and subsidences, together with associated changes in the obliquity of the ecliptic and the consequent variation in the intensity of seasonal changes, could afford an adequate explanation of the glacial epochs, though the best physical opinion was opposed to this view.

Darwin attacked this problem in characteristic manner. Not content with the mere qualitative indications of analysis his aim was to subject the results which might arise therefrom to definite numerical tests, and though the vagueness of the geological evidence available was such as to preclude any great precision in the results, the conclusions arrived at are quantitatively such as to be "absolutely inconsistent with the sensational speculations as to the causes and effects of the glacial epoch which some geologists have permitted themselves to make."

This paper was referred by the Society to Sir William Thomson (Lord Kelvin), and in this manner was the means of bringing Darwin into association with him. The acquaintance thus formed ripened into a close and intimate friendship which lasted till Kelvin's death. Henceforth we find the well known inspiring influence of Lord Kelvin pervading Darwin's work, and important memoirs followed one another in quick succession.

The conclusions arrived at in the paper referred to above were based on the assumption that throughout geological history, apart from slow geological changes, the Earth would rotate sensibly as if it were rigid. It is shown that a departure from this hypothesis might possibly account for considerable excursions of the axis of rotation within the Earth itself, though these would be improbable, unless, indeed, geologists were prepared to abandon the view "that where the continents now stand they have always stood"; but no such effect is possible with respect to the direction of the Earth's axis in space. Thus the present condition of obliquity of the Earth's equator could in no way be accounted for as a result of geological change, and a further cause had to be sought. Darwin foresaw a possibility of obtaining an explanation in the frictional resistance to which the tidal oscillations of the mobile parts of a planet must be subject. The investigation of this hypothesis gave rise to a remarkable series of papers of far reaching consequence in theories of cosmogony and of the present constitution of the Earth.

In the first of these papers, which is of a preparatory character, "On the Bodily Tides of Viscous and Semi-elastic Spheroids, and on the Ocean Tides on a Yielding Nucleus" ('Phil. Trans.,' 1879, vol. 170), he adapts the analysis of Sir William Thomson, relating to the tidal deformations of an elastic sphere, to the case of a sphere composed of a viscous liquid or, more generally, of a material which partakes of the character either of a solid or a fluid according to the nature of the strain to which it is subjected. For momentary deformations it is assumed to be elastic in character, but the elasticity is considered as breaking down with continuation of the strain in such a manner that under very slow variations of the deforming forces it will behave sensibly

as if it were a viscous liquid. The exact law assumed by Darwin was dictated rather by mathematical exigencies than by any experimental justification, but the evidence afforded by the flow of rocks under continuous stress indicates that it represents, at least in a rough manner, the mechanical properties which characterise the solid parts of the Earth.

The chief practical result of this paper is summed up by Darwin himself by saying that it is strongly confirmatory of the view already maintained by Kelvin that the existence of ocean tides, which would otherwise be largely masked by the yielding of the ocean bed to tidal deformation, points to a high effective rigidity of the Earth as a whole. Its value, however, lies further in the mathematical expressions derived for the reduction in amplitude and retardation in phase of the tides resulting from viscosity which form the starting point for the further investigations to which the author proceeded.

The retardation in phase or "lag" of the tide due to viscosity implies that a spheroid as tidally distorted will no longer present a symmetrical aspect as viewed from the disturbing body which generates the tides, as it would do if no such cause were operative. The attractive forces on the nearer and more distant parts will consequently form a non-equilibrating system with resultant couples tending to modify the state of rotation of the spheroid about its centre of gravity. The action of these couples, though exceedingly small, will be cumulative with lapse of time, and it is their cumulative effects over long intervals which form the subject of the next paper, "On the Precession of a Viscous Spheroid and on the Remote History of the Earth" ('Phil. Trans,' 1879, vol. 170, Part II, pp. 447-530). The case of a single disturbing body (the Moon) is first considered, but it is shown that if there are two such bodies raising tidal disturbances (the Sun and Moon)*the conditions will be materially modified from the superposed results of the two disturbances considered separately. Under certain conditions of viscosity and obliquity the obliquity of the ecliptic will increase, and under others it will diminish, but the analysis further yields "some remarkable results as to the dynamical stability or instability of the system . . . for moderate degrees of viscosity, the position of zero obliquity is unstable, but there is a position of stability at a high obliquity. For large viscosities the position of zero obliquity becomes stable, and (except for a very close approximation to rigidity) there is an unstable position at a larger obliquity, and again a stable one at a still larger one."

The reactions of the tidal disturbing force on the motion of the Moon are next considered, and a relation derived connecting that portion of the apparent secular acceleration of Moon's mean motion, which cannot be otherwise accounted for by theory, with the heights and retardations of the several bodily tides in the Earth. Various hypotheses are discussed, but with the conclusion that insufficient evidence is available to form "any estimate having any pretension to accuracy . . . as to the present rate of tidal friction."

But though the time scale involved must remain uncertain, the nature of the physical changes that are taking place at the present time is practically free from obscurity. These involve a gradual increase in the length of the day, of the month, and of the obliquity of the ecliptic, with a gradual recession of the Moon from the Earth. The most striking result is that these changes can be traced backwards in time until a state is reached when the Moon's centre would be at a distance of only about 6000 miles from the Earth's surface, while the day and month would be of equal duration, estimated at about 5 hours 36 minutes. The minimum time which can have elapsed since this condition obtained is further estimated at about 54 million years. This leads to the inevitable conclusion that the Moon and Earth at one time formed parts of a common mass and led to an inquiry as to how and why the planet broke up. The most probable hypothesis appeared to be that, in accordance with Laplace's nebular hypothesis, the planet, being partly or wholly fluid, contracted, and thus rotated faster and faster, until the ellipticity became so great that the equilibrium was unstable.

The tentative theory put forward by Darwin, however, differs from the nebular hypothesis of Laplace in the suggestion that instability might set in by the rupture of the body into two parts rather than by the casting off of a ring of matter, somewhat analogous to the ring of Saturn, to be afterwards consolidated into the form of a satellite.

The mathematical investigation of this hypothesis forms a subject to which Darwin frequently reverted later, but for the time he devoted himself to following up more minutely the motions which would ensue after the supposed planet, which originally consisted of the existing Earth and Moon in combination, had become detached into two separate masses. In the final section of a paper "On the Secular Changes in the Elements of the Orbit of a Satellite revolving about a Tidally Distorted Planet" ('Phil. Trans.,' 1880, vol. 171), Darwin summarises the results derived in this and preceding memoirs. Various factors ignored in the earlier investigations, such as the eccentricity and inclination of the lunar orbit, the distribution of heat generated by tidal friction and the effects of inertia, were duly considered and a complete history traced of the evolution resulting from tidal friction of a system originating as two detached masses nearly in contact with one another and rotating nearly as though they were parts of one rigid body. Starting with the numerical data suggested by the Earth-Moon system, "it is only necessary to postulate a sufficient lapse of time, and that there is not enough matter diffused through space to resist materially the motions of the Moon and Earth," when "a system would necessarily be developed which would bear a strong resemblance to our own." "A theory, reposing on *veræ causæ*, which brings into quantitative correlation the lengths of the present day and month, the obliquity of the ecliptic, and the inclination and eccentricity of the lunar orbit, must, I think, have strong claims to acceptance."

Confirmation of the theory is sought and found, in part at least, in the

case of other members of the solar system which are found to represent various stages in the process of evolution indicated by the analysis.

The application of the theory of tidal friction to the evolution of the Solar System and of planetary sub-systems other than the Earth-Moon System is, however, reconsidered later, "On the Tidal Friction of a Planet attended by Several Satellites, and on the Evolution of the Solar System" ('Phil. Trans.,' 1882, vol. 172). The conclusions drawn in this paper are that the Earth-Moon system forms a unique example within the Solar System of its particular mode of evolution. While tidal friction may perhaps be invoked to throw light on the distribution of the satellites among the several planets, it is very improbable that it has figured as the dominant cause of change in any of the other planetary systems or in the Solar System itself.

These researches were followed by a further application of Lord Kelvin's analysis of the strain in an elastic sphere to the determination of the strength of the materials of which the Earth must be built so as to prevent the continents from sinking and the sea-bed from rising—"On the Stresses caused in the Interior of the Earth by the Weight of Continents and Mountains" ('Phil. Trans.,' 1882, vol. 173). In this paper it is conclusively shown that the surface inequalities of the Earth's surface must give rise to enormous stress even at considerable depths comparable with the dimensions of the globe itself, and the continued resistance to this stress must imply a strength of material at least equivalent to that of granite. If this resistance is located in the surface layers only a still higher and almost inconceivable degree of strength will be indicated. Thus additional evidence is afforded by the dimensions of superficial inequalities of the Earth's crust of the solid structure of the Earth advocated by Lord Kelvin.

Simultaneously with the above researches we find Darwin, in conjunction with his brother, Mr. Horace Darwin, conducting experiments, in accordance with a suggestion of Lord Kelvin, for the purpose of directly measuring the deflections of the plumb-line due to the disturbing action of the Moon. The experiments failed in their purpose, not from want of delicacy of the apparatus used, but from the existence of more pronounced disturbing influences at the time but little understood, but which nowadays form the subject of continued observation by seismologists. The experiments afford an early contribution to the scientific study of these seismic disturbances; the separation of the lunar disturbances from them has since been successfully accomplished by Hecker.

The application of the theoretical researches on tidal friction to the consideration of the present structure of the Earth demanded observational data to be derived from existing knowledge of ocean tides. As the principal tides might be expected to depart far from the equilibrium law, and an adequate dynamical theory, on account of the complex distribution of the ocean, appeared to be far beyond the possibilities of mathematical analysis, such evidence as was required had to be sought in the separation from the records of tidal observation of the tides of long period which might be expected to

follow more closely the equilibrium law. This separation required a close and delicate analysis.

The theory underlying this analysis had been already laid down by Laplace, who had shown how the disturbing force may be analysed into various simple harmonic constituents, each of which will generate a tide of the same period as the disturbing force, the determination of the amplitude and phase of which, though not yielding to theory, can be effected for each port by direct observation.

The practical application of this theory to the discussion of tidal observations by means of harmonic analysis had been suggested by Lord Kelvin, and reports on the subject had been drawn up by him and presented to the British Association in 1868, 1870, 1871, 1872, and 1876. The whole subject was, however, in need of co-ordination and revision, and for this purpose a committee, consisting of Prof. Adams and Darwin, was appointed in 1882. The work of this committee devolved principally on Darwin, who, however, acknowledges the great benefit derived from the advice received from Adams from time to time. The output of this committee consists of a series of reports, dealing in a most thorough and complete manner with the co-ordination of the various existing methods of discussion of tidal observations, the derivation of harmonic constants with the highest degree of precision of which the observations permit, and the utilisation of these constants for the formation of tide tables. The schemes put forward by Darwin in these reports, of which a further account is given at length in the article "Tides," written by him for the 'Encyclopædia Britannica' in 1888, have since been practically universally adopted.

Following on this, we find Darwin turning his attention back to the problems arising in connection with the genesis of the Moon, in accordance with the indications previously arrived at from the theory of tidal friction. It appeared to be of interest to trace back the changes which would result in the figures of the Earth and Moon, owing to their mutual attraction, as they approached one another. The analysis is confined to the consideration of two bodies supposed constituted of homogeneous liquid. At considerable distances the solution of the problem thus presented is that of the equilibrium theory of the tides, but, as the masses are brought nearer and nearer together, the approximations available for the latter problem cease to be sufficient. Here, as elsewhere, when the methods of analysis could no longer yield algebraic results, Darwin boldly proceeds to replace his symbols by numerical quantities, and thereby succeeds in tracing, with considerable approximation, the forms which such figures would assume when the two masses are nearly in contact. He even carries the investigation farther, to a stage when the two masses in part overlap. The forms obtained in this case can only be regarded as satisfying the analytical, and not the true physical conditions of the problem, as, of course, two different portions of matter cannot occupy the same space. They, however, suggest that, by a very slight modification of the conditions, a new form could be found, which

would fulfil all the conditions, in which the two detached masses are united into a single mass, whose shape has been variously described as resembling that of an hour-glass, a dumb-bell, or a pear. This confirms the suggestion previously made that the origin of the Moon was to be sought in the rupture of the parent planet into two parts, but the theory was destined to receive a still more striking confirmation from another source.

While Darwin was still at work on the subject, there appeared the great memoir by M. Poincaré, "Sur l'équilibre d'une masse fluide animée d'un mouvement de rotation" ('Acta Math.,' vol. 7).

The figures of equilibrium known as Maclaurin's spheroid and Jacobi's ellipsoid were already familiar to mathematicians, though the conditions of stability, at least of the latter form, were not established. By means of analysis of a masterly character, Poincaré succeeded in enunciating and applying to this problem the principle of exchange of stabilities. This principle may be briefly indicated as follows: Imagine a dynamical system such as a rotating liquid planet to be undergoing evolutionary change such as would result from a gradual condensation of its mass through cooling. Whatever be the varying element to which the evolutionary changes may be referred, it may be possible to define certain relatively simple modes of motion, the features associated with which will, however, undergo continuous evolution. If the existence of such modes has been established, M. Poincaré shows that the investigation of their persistence or "stability" may be made to depend on the evaluation of certain related quantities which he defines as coefficients of stability. The latter quantities will be subject to evolutionary change, and it may happen that in the course of such change one or more of them assumes a zero value. Poincaré shows that such an occurrence indicates that the particular mode of motion under consideration coalesces at this stage with some other mode which likewise has a vanishing coefficient of stability. Either mode will, as a rule, be possible before the change, but whereas one will be stable the other will be unstable. The same will be true after the change, but there will be an interchange of stabilities, whereby that which was previously stable will become unstable, and *vice versa*. An illustration of this principle was found in the case of the spheroids of Maclaurin and the ellipsoids of Jacobi. The former in the earlier stages of evolution will represent a stable condition, but as the ellipticity of surface increases a stage is reached where it ceases to be stable and becomes unstable. At this stage it is found to coalesce with Jacobi's form which involves in its further development an ellipsoid with three unequal axes. Poincaré shows that the latter form possesses in its earlier stages the requisite elements of stability, but that these in their turn disappear in the later developments. In accordance with the principle of exchange of stabilities laid down by him, the loss of stability will occur at a stage where there is coalescence with another form of figure, to which the stability will be transferred, and this form he shows at its origin resembles the pear which had already been indicated by Darwin's investigation. The supposed pear-shaped figure was thus arrived

at by two entirely different methods of research, that of Poincaré tracing the processes of evolution forwards and that of Darwin proceeding backwards in time.

The chain of evidence was all but complete; it remained, however, to consider whether the pear-shaped figure indicated by Poincaré, stable in its earlier forms, could retain its stability throughout the sequence of changes necessary to fill the gap between these forms and the forms found by Darwin.

In later years Darwin devoted much time to the consideration of this problem. Undeterred by the formidable analysis which had to be faced, he proceeded to adapt the intricate theory of Ellipsoidal Harmonics to a form in which it would admit of numerical application, and his paper on "Ellipsoid Harmonic Analysis" ('Phil. Trans.,' A, 1901, vol. 197), apart from the application for which it was designed, in itself forms a valuable contribution to this particular branch of analysis. With the aid of these preliminary investigations he succeeded in tracing with greater accuracy the form of the pear-shaped figure as established by Poincaré, "On the Pear-shaped Figure of Equilibrium of a Rotating Mass of Liquid" ('Phil. Trans.,' A, 1901, vol. 198), and, as he considered, in establishing its stability, at least in its earlier forms. Some doubt, however, is expressed as to the conclusiveness of the argument employed, as simultaneous investigations by M. Liapounoff pointed to an opposite conclusion. Darwin again reverts to this point in a further paper "On the Figure and Stability of a Liquid Satellite" ('Phil. Trans.,' A, 1906, vol. 206), in which is considered the stability of two isolated liquid masses in the stage at which they are in close proximity, *i.e.*, the condition which would obtain, in the Earth-Moon System, shortly after the Moon had been severed from the Earth. The ellipsoidal harmonic analysis previously developed is then applied to the determination of the approximately ellipsoidal forms which had been indicated by Roche. The conclusions arrived at seem to point, though not conclusively, to instability at the stage of incipient rupture, but in contradistinction to this are quoted the results obtained by Jeans, who considered the analogous problems of the equilibrium and rotation of infinite rotating cylinders of liquid. This problem is the two-dimensional analogue of the problems considered by Darwin and Poincaré, but involves far greater simplicity of the conditions. Jeans finds solutions of his problem strictly analogous to the spheroids of Maclaurin, the ellipsoids of Jacobi, and the pear of Poincaré, and is able to follow the development of the latter until the neck joining the two parts has become quite thin. He is able to establish conclusively that the pear is stable in its early stages, while there is no evidence of any break in the stability up to the stage when it divides itself into two parts.

Reference must now be made to Darwin's work on the subject of "Periodic Orbits." Though no published work on this subject appeared before the year 1897, the memoir which then appeared contained the substance of work which had occupied him for some years previously, the continuation of which only ceased with his death. The work had its origin in the beautiful

memoirs of Mr. G. W. Hill on the Lunar Theory. The usual method of procedure in discussing "the problem of three bodies" is to base the solution on the "problem of two bodies," *i.e.*, on the theory of elliptic motion, and then to calculate by successive approximations the small disturbances resulting from the presence of a third body. Hill was the first to show that certain special solutions of a simple character could be derived which presented marked superiority over the elliptic orbits previously used as a starting point for more exact investigation. As applied to the Lunar Problem he succeeded in determining by analytical methods a solution in which all those inequalities (the variational inequalities) dependent on the ratios of the mean motions of the Sun and Moon, but independent of the eccentricity and inclination of the lunar orbit and of the Sun's parallax, are taken into account at the outset. Owing to the slow convergence of the series involved, the analytical methods fail when the ratio of the month to the year is increased much beyond the value which actually holds, but Hill showed that in such cases the special solutions could still be derived by a method of numerical quadrature.

The initial object of Darwin's research was to apply Hill's method of investigation to cases which departed somewhat widely from the traditional cases dealt with in the lunar and planetary theories, and where strictly analytical methods were of little avail. He therefore adopts the method of numerical quadratures from the outset. The problem which he set himself was to trace out the possible paths of a small body (or satellite) moving in the plane of the circular orbit of a planet (Jove) round the Sun; from among such possible paths he then sought to pick out, by trial and error, the particular ones which fulfilled the condition of Hill's lunar orbit, *viz.*, that after the lapse of a certain interval the conditions which obtained at the commencement of the interval would be exactly reproduced, so that the solution obtained would be "periodic" in character. Thus, it would only be necessary to investigate the features pertaining to a single period to obtain a knowledge of the motion of the satellite for all time.

In order to emphasise the phenomena of perturbation, Darwin started with a case where the mass of the planet was considerable compared with that of the Sun. The actual numerical value adopted for the ratio of the masses of the planet and Sun was 1:10, and this was adhered to throughout. The differential equations of motion admit of one integral, Jacobi's integral, which introduces an arbitrary constant (C), the constant of relative energy. It was found convenient to classify the orbits in accordance with the value of this constant.

Following Hill, Darwin shows that for large values of C , the orbits described will all be contained either within a closed curve surrounding the planet, within a similar closed curve surrounding the Sun, or outside a closed curve which surrounds both of the former. The three cases correspond with the lunar theory, planetary theory as applied to an inferior planet, and planetary theory as applied to a superior planet.

For smaller values of C , however, the different branches of these limiting curves unite, and passages are opened up through which a satellite may be transferred from one of the spaces to another. The great point of interest was to investigate the features associated with such a transference, and consequently the investigation was limited to the smaller values of C which would permit of this possibility. Even with the further limitation that "simply" periodic orbits alone (*i.e.*, those which repeat themselves after a single revolution round the Sun, or primary) were considered, the amount of work required was found to be prodigious. The interest in the subject was sustained by the continued surprises which the results yielded, and he was thus induced to continue computing more and yet more orbits whose forms appeared to be typical. Many of these were of a highly complex character, the arithmetical determination in such cases being almost always highly evasive.

Not content with merely indicating the forms of these orbits, he set himself in every case the still more difficult task of discussing their stability. In order that a satellite may describe a periodic orbit it must satisfy ideal initial conditions, any departure from which will cause it to describe initially a closely adjacent orbit. For certain orbits the disturbed orbit will oscillate in relation to the periodic orbit in a period which is associated only with the properties of the latter orbit, and is independent of the nature of the disturbance, provided only the latter be small. This was the case with Hill's variational orbit, but in other instances an alternative presents itself in which the quantity, which figures analytically as the period of the disturbance, presents itself as an imaginary or complex quantity. In such a case the amplitude of the oscillations will increase with greater or less rapidity, and the disturbed orbit will soon cease to follow even approximately the course of the periodic orbit. In the latter case the periodic orbit is said to be unstable.

The problem of determining the periods of the small oscillatory disturbances, whether of real or imaginary period, is identical with that dealt with by Hill in his determination of the motion of the lunar perigee. Darwin at first followed Hill's methods, in which the solution is derived by the reduction of a determinant of infinite order, but later an alternative method depending on quadratures was devised. But whichever plan was used the computations were found to be exceedingly laborious, and for orbits with sharp flexures almost intractable. Nevertheless, in almost every case he is able to arrive at a definite conclusion as to the stability or otherwise of the orbits traced.

The subject of these investigations was dealt with simultaneously by Poincaré in his volumes dealing with "*Les Nouvelles Méthodes de la Mécanique Céleste*." Both authors derived their inspiration from Hill's work, but the methods of treatment differ as widely as do their respective methods of treatment of the problem of the figures of equilibrium of rotating fluid. Poincaré's method consists in a discussion of the analytical

properties of the fundamental differential equations of dynamics, and from his vast knowledge of the theory pertaining to such equations to deduce properties of their solutions without actually deriving the solutions. However much one may admire Poincaré's skill and insight in dealing with abstruse mathematical analysis, to many readers the abstract process of reasoning will be found unsatisfying, and more concrete presentations of the problems will appear to possess greater lucidity. To all the actual derivation of definite solutions of the problem of three bodies by a process which admits of wide extension will appear as a high achievement. Many will share Darwin's own distrust, based on experience, of the possibility of correctly reasoning without fear of ambiguity about solutions which have not been definitely found, but his own results cannot fail to carry conviction. Darwin's orbits give clear and tangible illustrations of many of the features derived by Poincaré through the medium of his analysis, but from the nature of his methods, which consist in the study of special cases, they do not permit of such wide generalisation. Thus the work of either investigator in a field which, previously to the publication of Hill's great memoir, had come to be considered as practically inaccessible, must be regarded as complementary to that of the other.

We have confined ourselves hitherto to a summary of the more important researches which occupied Darwin's attention. The subjects of these researches were frequently dealt with in his professorial lectures, which, whether he was expounding his own work or that of others, were always marked by singular lucidity. The current topics of investigation were almost always introduced in these lectures and discussed with his students. He was conscientious to a degree in acknowledging any assistance he himself derived in this manner and whether in the lecture room or in his private study was eager to discuss projects for research on collateral subjects. His own enthusiasm communicated itself to his pupils and his influence did much to maintain the traditional reputation of Cambridge as a school of applied mathematics, and this in spite of the fact that, with the growing need for increased specialisation, a strong tendency existed for the separation of the subjects of study into two distinct schools, that of pure mathematics and that of experimental physics.

In University administration he took but little part in internal politics, but in matters concerning the relationship of the University with external bodies his influence was great and his services were always available. In the formation of the Cambridge University Association for the purpose of raising increased funds for University purposes he took a leading part, as also in the scheme for an Employment Bureau to bring University candidates more prominently into touch with the various openings that presented themselves in after walks of life on completion of their University course.

Outside the University his services were in demand in connection with the various learned societies with which he was associated.

For the Royal Society he frequently served on Committees and officiated as a referee for numerous papers. He was President of the Royal Astronomical Society in the year 1889–1900, when it devolved on him to deliver an appreciative address on delivering the Gold Medal of the Society to his famous co-worker Poincaré. This address has been referred to by those who were privileged to hear it as one of the most inspiring that has ever been heard from that Chair on a similar occasion.

An event of great importance in his life was the occasion on which he was invited to occupy the Presidential Chair of the British Association in 1905, on the occasion of its visit to South Africa. The task was an exceptionally difficult one, involving, besides the delivery of two formal addresses at the two more important centres visited, innumerable minor speeches at almost every place of call *en route*; in each one of which he was exceedingly happy in adapting himself to the occasion. He took no small share in the preliminary organisation for the journey of the Society, and it was largely due to his personality and tact in adjusting minor differences that the arrangements proved so efficient and frictionless in actual operation. His Presidential Address on this occasion deals with a remarkable analogy between the subjects of his own investigations and collateral investigations in biological and political science. On his return to England he received the well-deserved honour of a K.C.B. at the hands of His Majesty.

In 1897 he was invited to America to deliver a course of lectures at Boston and chose as his subject “The Tides.” These lectures formed the nucleus of a volume published by him in 1898 under the title of ‘The Tides and Kindred Phenomena of the Solar System,’ in which a semi-popular account is given of many of his important researches. The book met with a hearty reception and has since passed through many editions and been translated into many foreign languages.

He was nominated as a member of the Meteorological Council soon after his return to Cambridge, and continued to serve as a representative of the Royal Society on the Treasury Committee which superseded that Council a few years ago.

He was appointed by the Foreign Office as the first British representative on the International Geodetic Association, a position which involved him in extensive correspondence with the various geodetic organisations throughout the British Dominions. The choice is admitted by all to have proved an exceedingly happy one, and his services were duly acknowledged by the Association itself when they accepted the invitation, conveyed by him, to hold their triennial meeting in England in 1909, and nominated him as President for the occasion.

He took a leading part in the organisation of the meeting of the fifth International Congress of Mathematicians, which was held at Cambridge on August 22–28 of last year, and, in spite of failing health at the time, fulfilled the duties of the presidency with notable success. The symptoms were, unfortunately, to prove fatal, and, after a protracted illness, he passed

peacefully away on December 7 last, to the great sorrow of all who were privileged to know him.

He was a corresponding member of many learned Societies, both in Europe and America, and many honours were conferred on him, in appreciation of his scientific work. The Gold Medal of the Royal Astronomical Society was awarded to him in 1892, and a Royal Medal on the nomination of the Royal Society in 1884, while, shortly before his death, he had the gratification of receiving the greatest mark of distinction which the Society can confer, by the award of the Copley Medal in the year 1911, and in 1912 the Victoria Medal of the Royal Geographical Society.

In private life his characteristic energy showed itself in a multitude of ways, such as in the mass of miscellaneous knowledge he had acquired from books, and in his facility in languages. As a trifling instance may be mentioned his acquaintance with heraldry, in which he had grounded himself as a little boy at Down, daily poring over the abstruse works of Guillim and Edmondson. The careful drawings made from these books doubtless trained his powers of draughtsmanship, which in later life were shown in his illustrations of some of his father's works.*

The same trait might be illustrated in many ways, *e.g.* in the zeal with which as a boy he collected lepidoptera, or the vigour with which as a young man he mastered the difficult game of tennis, just failing, however, to represent his University against Oxford; or again, near the end of his life, in his patient attempt to become an archer. The holidays of life, and especially the pleasures and amusements of his wife and children, were shared by him, and organised with a happy and rapid effectiveness.

Here, and indeed in every relation of life, his energy was coloured and made lovable by that simple sweet open nature which endeared him to so many.

S. S. H.

* He is mentioned in the 'Fertilisation of Orchids' as having solved the problem of the Musk Orchis (*Herminium monorchis*).

SIR WILLIAM WHITE, 1845-1913.

SIR WILLIAM WHITE died unexpectedly on February 27 last, while engaged in London on his ordinary everyday work, aged 68 years. The world is a loser by his death. He was an ardent worker in many fields, in all of which he gained distinction.

He was born at Devonport in 1845, his father being a native of that town and his mother a native of Lostwithiel. He was the youngest child of a large family, and was given no special educational facilities. Writing of this time afterwards he says: "I had to travel a hard road."

After studying in a small private school in Devonport, where he made the best use of such opportunities as the place afforded, he left it as head boy to become a shipwright apprentice in Devonport Dockyard.

The Admiralty—as Sir William White recalled in his Chairman's Address to the Royal Society of Arts—led the way in the provision of technical education for the youth of this country; and it was in Devonport Dockyard School for Apprentices that Sir William may be said to have commenced that study of Naval Architecture which was to carry him so far.

Side by side with his purely school work, he was engaged at this period in the actual building and repairing of ships, and so acquired that combined scholastic and workshop knowledge of which he was so strong an advocate in later years. This is now generally known as the "sandwich system" of technical training.

In 1864, the Admiralty found themselves faced by a great emergency. The end of the wooden ship-of-war had come, and for the reconstruction of the Navy they needed a steady supply of officers thoroughly trained in the science of Naval Architecture. Under the advice of Mr. E. J. Reed (afterwards Sir E. J. Reed, K.C.B., F.R.S.) and the Council of the Institution of Naval Architects they reverted to a scheme which twice in the century they had abandoned, and they instituted at South Kensington Museum the Royal School of Naval Architecture and Marine Engineering. Sir William White was one of eight shipwright apprentices chosen for its first students. The Admiralty spared no expense to make the training thoroughly efficient, and obtained the services of eminent professors in all branches of science necessary for Naval Architects and Marine Engineers. Sir William was an eager student at this school, and obtained its highest diploma. When he left the school in 1867 he was appointed, with five other pupils, by Sir Edward Reed (the Chief Constructor of the Navy) to the Admiralty Staff for the design and building of H.M. ships. He assisted Sir Edward Reed personally in the preparation of his great work on 'Shipbuilding in Iron and Steel,' published in 1869, and in the preparation of the Memoir on the Stresses on Ships contributed by Sir Edward to the 'Philosophical Transactions' for 1871.

The problems of naval design at this period were numerous, and the most

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diverse views were held by those in authority. The main questions involved, so far as the ships of the line were concerned, were :—

1. What was the best method of constructing the armoured side of a ship so as to secure the greatest resistance to gunfire ?
2. What was the best method of disposing the area of the armour on a ship's side so as to secure a just balance between the protection, in action, of vital fighting parts, and the exclusion of water from other parts of the vessel, where its entry in excessive quantities might lead to the foundering of the ship ?
3. What was the best disposition of the armament ?
4. Whether, on the whole, it was best to build a short vessel like Sir Edward Reed's *Bellerophon* or a long vessel like the first armoured *Warrior* ?

Sir William took up the discussion of these points with such ability as to throw much light on the problems at issue. He also assisted Sir Edward Reed in preparing his book 'Our Ironclad Ships,' which dealt *in extenso* with the question of short *v.* long ship.

In 1870, Sir Edward Reed retired from the position of Chief Constructor of the Navy. The office was then put into Commission, with Sir Nathaniel Barnaby, K.C.B. (then Mr. Barnaby), as Chairman of the Council of Naval Construction, and so well was Sir William's work appreciated by the new head, that he made him Secretary of the Council.

In January, 1871, Lord Dufferin's Committee was appointed "To examine the Designs upon which Ships-of-War have recently been constructed," with special reference to the loss of the *Captain*, and a large calculating staff, composed wholly of men trained at the Royal School of Naval Architecture and Marine Engineering, was formed at the Admiralty under Mr. F. K. Barnes, Member of the Council of Construction, for the purpose of preparing data for the Committee. Numerous calculations concerning the stability, strength, and other features of existing ships were demanded, and these had to be much more thorough in character than was at that time customary. In this work Sir William took a prominent part.

During this early period of his career Sir William had already shown that devotion to the literature of his profession which was a prominent feature of his career. The first volume of the Annual of the Royal School of Naval Architecture and Marine Engineering was published in 1871 under the direction of a Committee of which Sir William was a member, and although there is no signed paper of his in the volume, he was a large contributor to its contents. In 1871, he, in conjunction with Mr. W. John, submitted a paper to the Institution of Naval Architects on "The Calculation of the Stability of Ships, and some matters of interest connected therewith." This was the first of many papers which he contributed to the Institution of Naval Architects. They were always of first-rate importance. Many of them dealt with matters of Naval policy as distinct from Naval Architecture, and the views which he

put forward were always received with great respect. Throughout his whole life he took a vigorous part in the discussion of papers at the Institution, and so late as last year he made a valuable contribution to the discussion of geared turbines and oil engines.

From the beginning of his services at the Admiralty, Sir William much interested himself in the education of young naval architects, and in 1869 he was appointed to succeed his former teacher Mr. Crossland, Member of the Council of Construction, and a distinguished member of an earlier school of Naval Architecture, as Lecturer on Naval Design at the South Kensington School. This position he retained for some years after the transfer in 1873 of the School to the Royal Naval College, Greenwich.

While holding his position, he assisted Dr. T. Archer Hirst, F.R.S., the Director of Studies of Greenwich, in arranging a course of instruction in Naval Architecture for the benefit of executive Naval officers. The syllabus of instruction was so well chosen and so wisely carried out, largely under his guidance, that large numbers of officers were attracted to the classes, which continue in effective operation to the present time.

About this time he also put forward a well considered scheme for the formation of a Royal Corps of Naval Constructors, which was to replace the heterogeneous system then in force. This was adopted by Sir Nathaniel Barnaby and by Sir W. Houston Stewart, K.C.B., the then Controller of the Navy. After consideration on their part, a Committee was appointed, with Sir T. Brassey, now Earl Brassey, as President, to consider this scheme, and in 1883 the Crown, by Order in Council, created the Corps on its existing footing.

Sir William was promoted to the rank of Constructor in 1875, and to that of Chief Constructor in 1881.

The chief designing work on which he was engaged under Sir Nathaniel Barnaby in 1876-77 was that of the famous *Inflexible*, which had two turrets *en échelon*, each containing two 16-inch muzzle-loading guns. The turrets themselves were enclosed in a comparatively short armoured central citadel, and the ends of the ship were unprotected by vertical armour, having armoured decks only.

The design of this vessel excited very strong adverse criticism, led by Sir Edward Reed. A specially competent Committee, consisting of Admiral Sir James Hope, G.C.B., Dr. J. Woolley, Mr. G. W. Rendel, and Mr. W. Froude, F.R.S., was appointed to report on the stability of the vessel, and after long and exhaustive investigation, made both at sea on actual ships, and with models in the experimental tank of Mr. William Froude, the Committee reported that the design fully satisfied the desired conditions. This design was repeated on a smaller scale in the *Ajax* and *Agamemnon*, and in the two somewhat larger vessels *Colossus* and *Edinburgh*, although even these were still much smaller than the *Inflexible*.

Other vessels on which Sir William was engaged during this period, under Sir Nathaniel Barnaby, were the cruiser *Iris* (the first steel vessel built for the Royal Navy), the *Mersey* class of cruiser, and the *Admiral* class of

battleship. The *Admiral* class excited fierce criticism on account of their so-called "soft ends," and this criticism did not cease till some time after Sir William's return to the Admiralty in 1885.

From 1883 to 1885, Sir William was engaged as Warship Designer and Manager of the War-Shipbuilding Branch of Messrs. Sir W. G. Armstrong, Whitworth and Co., at Elswick-on-Tyne. He there laid out the new shipyard at Elswick and designed and laid down several vessels for Foreign Powers.

In 1885 he was offered by Lord George Hamilton, then First Lord of the Admiralty, the position of Assistant Controller of the Navy and Director of Naval Construction in succession to Sir N. Barnaby, who had retired on account of ill health, and he accepted the offer and returned to the Admiralty.

At this time some of the vessels of the *Admiral* class were still in course of construction. There were also building the *Victoria* and *Sanspareil* (the former at Elswick Shipyard under his directions), the *Trafalgar*, *Nile*, and *Hero* battleships; the *Impérieuse* and *Warspite*, and the seven vessels of the *Orlando* battle-cruiser class; the *Rattlesnake*, the first of our so-called torpedo gunboats and the forerunner of our torpedo boat destroyers, was under construction.

Sir William at once proceeded to take up the work of developing naval design, so as to employ to the greatest advantage the material available for attack and for defence.

As regards the cruiser class of vessel, he adhered for some time to the system, first introduced in the Elswick-built Chilean cruiser *Esmeralda* (designed by Mr. George Rendel), of a strong arched steel deck, rising, at the middle line of the ship, somewhat above the water line, and pitched at some feet below the water line where it reached the side of the ship. This system of protection, in conjunction with large coal bunkers flanked on the inside with cofferdams, gave on the whole, he considered, the best protection available under cruiser conditions as then laid down. In this view the Board of Admiralty fully concurred, although the absence of side armour did not escape criticism.

This type of vessel reached its culmination as regards size in the *Powerful* and *Terrible*, and, although side armour had been adopted by other countries for such vessels, it was not till substantial improvements had been made in the quality of armour, that Sir William recommended the Board (in June, 1897) to adopt side armour protection for our cruisers. This led to the laying down of six vessels of the *Cressy* class, followed by four vessels of the *Drake* class, and 10 somewhat smaller vessels of the *Monmouth* class.

As regards smaller cruisers, Sir William designed and built the Australian cruisers of the *Wallaroo* class, and the *Medea*, *Barrosa*, and *Barham* classes for the Royal Navy.

He made improvements in the design of the torpedo gunboat *Rattlesnake* and produced the *Sharpshooter* and later vessels, which were of a much more habitable type than had previously prevailed.

Sir William's first battleships were the eight vessels of the *Royal Sovereign* class, a development of the *Trafalgar* class in which modifications of the dimensions and the disposition of armament were introduced. The *Royal Sovereign*, the first vessel of the class, was built in less than three years and was completed in 1892.

After close comparative study of the arrangements of armament and protection found in foreign warships then building, and of the rapidly changing engineering factors of the date, he was in a position to make a special analysis of the real elements of fighting efficiency. In 1889, with the sanction of the Admiralty, he read a paper before the Institution of Naval Architects giving his views on the general constructive position, and setting forth the considerations which had influenced the Board in ordering the *Royal Sovereign* class. He was much criticised by some members of the Institution, but it was recognised that he made good the position he had taken up.

The main features of the *Royal Sovereign* class were followed in our own and some foreign navies up to the appearance of the *Dreadnought* in 1905.

During Sir William's tenure of the post of Director of Naval Construction, it was considered necessary by the then Board of Admiralty, owing to the political circumstances of the time (1889), for the Admiralty to make special building efforts. Under a loan embodied in the Naval Defence Act, 70 vessels of various classes were built, and Sir William White carried out his share of this work with complete success.

Sir William served under several successive Boards of Admiralty and received the fullest confidence of all.

Physically strong as he originally was, the work he had done from his appointment as Director of Naval Construction in 1885 gradually undermined his health; and in order to avoid a complete and possibly irremediable breakdown he felt compelled to retire in 1901.

When we consider his long period of service as Chief Constructor of the Navy, the special circumstances which necessitated changes in all types of ships, and the great pressure caused by the construction of the 70 special naval defence ships, his tenure of office must be regarded as highly distinguished. It is a record of which any man might be proud, and for which his country must be grateful.

He was an Honorary Vice-President of the Institution of Naval Architects, Past President of the Institution of Civil Engineers, Chairman of the Royal Society of Arts, and President of many other scientific societies; he was LL.D. of Glasgow, D.Sc. of Cambridge, Durham, and Columbia, was a member of many foreign societies, and held many other similar degrees and honours. He was a Past Master of the Worshipful Company of Shipwrights, and in his association with this Company he did much to promote the professional education of naval architects and marine engineers.

As a Fellow of the Royal Society he did much useful work in connection with the National Physical Laboratory, of the Governing Body and Executive Committee of which he was a member. One of his latest acts in connection

with the Laboratory was the securing of a large donation from the Drapers' Company on its behalf.

Sir William had very close connection with various City companies, so far as they assisted in education or the endowment of research, and freely gave his time to assist them in all such matters.

He was a most competent chairman of a meeting on any professional subject, and a ready speaker in general who had always something interesting and useful to say. He was especially expert in carrying a meeting past matters on which there was much discussion, into regions where unanimity prevailed and effective action could be taken. He was a splendid debater, and throughout the long controversy between Sir Edward Reed and the Admiralty as to its shipbuilding policy (especially in regard to the *Inflexible* type and the *Admiral* class of ship), his defence was recognised as brilliant and convincing and after the discussion on the *Royal Sovereign* class already referred to, all serious attacks upon the shipbuilding policy of the Admiralty disappeared, and the utmost confidence prevailed.

These notes have necessarily been confined to a consideration of Sir William White's career as a naval architect, but he was a man of far too wide an outlook to be contained by any limits of profession. After his integrity, ability, and perseverance, the quality which carried him farthest was perhaps the gift of speaking. Speaking was a pleasure to him. He was very fluent, but his words were always well chosen. He was equally good in setting out a proposition, in quick debate and repartee, and in an amusing after dinner speech. He had a great power of marshalling arguments, and of converting opponents by tactical insistence. He wrote much and his writing was unaffected, lucid, and convincing.

He was a man of high ideals and gave his services unsparingly to any deserving cause. He was a thoroughly religious man without being in any way a religionist. Wherever he went he carried with him a wholesome atmosphere. He was a kind and most helpful friend to numbers of young men.

Within a week of his death he wrote to a friend whom he had known for 30 years: "I fear that your recognition of those days puts too high a value upon any help that I then rendered you. But I do claim to have endeavoured all along, and still endeavour, to assist and encourage young men who are making similar efforts, and I have rejoiced greatly in your personal success."

In the annals of the British Navy, Sir William White's name will long be remembered with honour. His career will serve as an example to the Constructive Corps which he did much to originate and to train; and that Corps must always be a potent factor in the maintenance of the naval power of this country. And so we leave him, with a feeling of regret and gratitude for a kindly as well as clever man.

P. W.

SAMUEL ROBERTS, 1827–1913.

SAMUEL ROBERTS was the second son of the Rev. Griffith Roberts, Presbyterian minister, and of Anna, the eldest daughter of Samuel Churchill, a merchant of Exeter. He was born at Hackney, December 15, 1827.

He was educated at Queen Elizabeth's Grammar School, Horncastle, Lincs., his father being minister of the Presbyterian Chapel at Kirkstead, near Horncastle, and resident at Horncastle. He passed from the Grammar School to Manchester New College in June, 1844. He matriculated at the University of London in 1845 with Honours in Classics and Mathematics. He took the B.A. in 1847 with Honours in Mathematics, and the M.A. in 1849, when he was first in Mathematics and Natural Philosophy and obtained the Gold Medal.

He was admitted a solicitor in 1853, having served his articles of clerkship with Mr. Richard Mason, Town Clerk of Lincoln. After some years he gave up practice, removed to London and devoted himself to mathematical research.

He was elected a member of the London Mathematical Society in June, 1865, a few months after its foundation. He was Honorary Treasurer of that Society from 1878–1880, President, 1880–1882. He was elected a Fellow of the Royal Society in 1878, served on the Council of the London Mathematical Society for many years and was the de Morgan Medallist of 1896.

He was twice married; in 1858 to Mary Ann Astley, only child of the Rev. Richard Astley, formerly of Shrewsbury. She died in 1894. In 1896 he married Lucy Elizabeth Holland, second daughter of Philip Henry Holland, surgeon and Government Inspector, who survives him.

By his first marriage he had three sons. The eldest was Samuel Oliver Roberts, M.A., formerly scholar of St. John's College, Cambridge, 7th wrangler in 1882 and later Mathematical Master at Merchant Taylors' School. He died in 1899. The second son is Harry Astley Roberts, B.A. (Lond.), solicitor, who is still living. The third son died in infancy.

He died September 18, 1913, at the ripe age of 86, and was buried in Highgate Cemetery.

The Royal Society Catalogue of Scientific Papers shows that up to the year 1883 he contributed 62 papers to Mathematical Journals. These commenced in 1848 and for about ten years his preference led him to work at geometry and the Calculus of Operations. His most important paper at this time was "On the Transformation of Co-ordinates," published in the 'Quarterly Mathematical Journal' of 1858. The foundation of the London Mathematical Society in 1865 stimulated him to increased activity, and from that date to 1873 a long series of papers were given to the Society's Proceedings and to other journals. These are mostly concerned with geometry and include important contributions, of which may be noted "On the Ovals of Descartes,"

L.M.S., 1870, "On the Order and Singularities of the Parallel of an Algebraic Curve," L.M.S., 1871, "On the Motion of a Plane under certain Conditions," L.M.S., 1871, "On the Parallel Surfaces of Conicoids and Conics," L.M.S., 1872, "On Professor Cremona's Transformation between two Planes," L.M.S., 1872, "On Parallel Surfaces," L.M.S., 1873.

From the year 1875 he became interested also in the Theory of Numbers, and made some noteworthy contributions to the forms of numbers determined by continued fractions and to Euler's theorem on the product of two sums of four squares. His valedictory address to the London Mathematical Society upon vacating the chair was a masterly discourse entitled "Remarks on Mathematical Terminology and the Philosophic Bearing of recent Mathematical Speculations concerning the Realities of Space," 'L.M.S. Proc.', vol. 14. He has left a considerable mathematical correspondence with de Morgan, Salmon, Cayley, Sylvester and others, and was in intimate friendship with many mathematicians of his day such as Hirst, J. J. Walker, R. Harley, Spottiswoode and Perigal. He had many interests besides mathematics. From his earliest years he worked with the lathe and liked to construct electrical machines and other scientific apparatus. He was devoted to chess, angling and philosophy. He was interested in geology and microscopy and spent much of his leisure in collecting geological specimens for the microscope. He was for many years a member of the Quekett Microscopical Society. For the last ten years of his life he was almost totally blind. This infliction he bore with great patience. Throughout his life he was of a retiring disposition and regarded original research as being its own reward.

As an ardent worker, who made not a few important additions to mathematical science, he will not be soon forgotten.

P. A. M.

JOHN MILNE, 1850-1913.

JOHN MILNE, geologist, mining engineer, seismologist, and traveller; Honorary Fellow of King's College, London; F.G.S., D.Sc. (Oxon); Royal Medallist, 1908; Lyell Medallist of the Geological Society, 1894; was born at Liverpool December 30, 1850. He was the only child of John Milne, of Milnrow, Rochdale, and Emma, daughter of James Twycross, J.P., of Wokingham. He married Tone, daughter of Horikawa Noritsune, the high priest of Hakodate. After school at Rochdale and Liverpool he went to King's College, London. Gaining a scholarship he now attended the Royal School of Mines and studied Geology and Mineralogy under Prof. Sir Warrington Smyth. After some practical mining experience in Cornwall and Lancashire he studied Mineralogy at Freiberg and visited the principal mining districts of Germany. At the request of Mr. Cyrus Field he spent two years in investigating the mineral resources of Newfoundland and Labrador (see 'Jour. Geol. Soc.,' 1874; 'Geol. Mag.,' 1876 and 1877). He visited Funk Island, once the home of the Great Auk, and made a large collection of skeletons of that extinct non-flying bird which used to frequent the northern parts of Great Britain. He showed his fondness for travel very early, for at school, when he obtained a money prize just before the holidays, he started for Iceland without parental leave and made his first experience in a very dangerous exploration of the Vatna Jokul. At the much later date of 1874 he joined the expedition of Dr. Beke, which, under the auspices of the Royal Geographical Society, started to investigate what Dr. Beke thought to be the true site of Mount Sinai, in Arabia, east of the Gulf of Akaba (see 'Jour. Geol. Soc.,' 1875). Prof. Milne often spoke of the shallowness of the south end of the Gulf and of the possibility of its being crossed on foot if a strong north-east wind was blowing. About this time he published interesting geological notes on the environs of Cairo ('Geol. Mag.,' 1874).

In 1875 he was appointed Professor of Geology and Mining in the Imperial College of the Public Works Department, Tokio, Japan, and he spent eleven months in reaching Japan. He travelled through Siberia by tarantass and sledge to Kiachta, very much on the line now followed by the railway. He crossed Mongolia with a camel train in the depth of winter, the temperature being usually well below zero Fahrenheit ('Trans. Asiatic Soc. Japan'). His boyhood experience on the Vatna Jokul made this appear commonplace. He passed the Great Wall on January 11, 1876, and travelling through Pekin and Tientsin he reached Shanghai on February 24. In Japan he proved to be an excellent teacher. He made many tedious expeditions through Japan and Yezo studying their geology, but he soon studied little else than earthquakes. At that time, although much had been written about seismology, it was not a science; there were no instruments which

truly recorded the motion of the ground. In the course of time, by persistent experimenting, by taking everybody into his confidence and asking for help from all sorts of scientific experts, he invented good instruments and gained a wonderful knowledge of what we now call the science of seismology.

After the destructive earthquake of 1880 he arranged a public meeting which was well attended by Japanese and foreigners; the Seismological Society of Japan was formed at that meeting, with Prof. Milne as Secretary. For fifteen years he directed its work, the results of which are given in twenty volumes of reports, well known to seismologists. The Japanese Government established an Earthquake Committee, which it still supports with an annual subsidy, whose average amount is 10,000 dollars, but which is sometimes as great as 25,000. When he gave up his Professorship in Japan in 1895 he left behind him a well trained staff of seismologists and many observing stations. There are nearly 1000 observing stations now in the Japanese Empire. The Emperor conferred upon him a pension and the Order of the Rising Sun. As to the necessity of numerous observing stations, in 1883 his book on 'Earthquakes' states:—"It is not unlikely that every large earthquake might, with proper instrumental appliances, be recorded at any point on the land surface of our globe." It was some time before this surmise was proved to be true, but it is the foundation of all modern progress in seismology.

Soon after his return to England he settled at Shide, near Newport, in the Isle of Wight. Since that time, as secretary of a committee of the British Association, he has established about 60 observing stations, in selected positions, all over the world. Whatever other instruments there may be in those stations, each of them has one or more specimens of Milne's standard seismometers. Instructions and reports are sent out regularly from Shide to these stations; photographic records are regularly sent to Shide. These are all carefully compared with one another, and an elaborate annual report of the world's seismology is published by the British Association, whose grants to this committee since 1895 have often been as much as £130 per annum. From the Government Grant to the Royal Society it has received altogether £600. Mr. Matthew Gray, one of Milne's personal friends, gave, some years ago, a sum of £1000, which is invested. Prof. Milne's private expenditure on the work of the committee has always been considerable. He has left a bequest of £1000, to help in continuing the work, but this will not take effect till the death of Mrs. Milne. Prof. Judd was for many years the chairman of the committee; he retired in 1907, and Prof. Turner, of Oxford, became chairman. It is hoped that a fund may be formed to enable the work to be continued.

There is not only an observing station at Shide, remote from the dwelling-house, but also, attached to the house, a laboratory, which contains a library and a collection of records, and it is the business centre of administration. An account of the work done at Shide, written in 1912 by Mrs. Lou Henry Hoover, will be found in the 'Bulletin of the Seismological Society of America,' vol. 2, No. 1. Many such accounts have been published, for Shide had always

a great number of visitors, of all ranks and from all parts of the world, and Prof. Milne was very hospitable; but Mrs. Hoover gives the best account of how it was that everybody enjoyed and envied his enthusiasm in his work, and admired his unstinted praise for other workers, and the intense youthfulness of a man who was 62 years of age.

His numerous scientific papers show that he followed many side-tracks by persistent strenuous experiment and observation. One that he thought of great importance was the varying photographic effect obtainable from rock surfaces in caves that were light-proof. He saw some connection between this and the great health differences that exist between two neighbouring places. The movement of the ground due to change of tide, the approach of hills due to rainfall, the velocity of waves of the ocean produced by earthquakes, the fracture of submarine telegraph cables, the yielding of rock near places where there are sudden great differences in level, and many other problems, occupied his attention. He was never tired of trying the new instruments invented by himself and others. He instituted methods of building railway bridges and houses in countries subject to earthquakes. He induced railway companies to test the soundness of their tracks and bridges by instruments carried on the trains. The Colonial Office made use of his services, for when all the cables giving telegraphic communication with a colony were simultaneously broken, it was important to know whether the fracture was due to an earthquake or to the machinations of a foreign power. Insurance offices made use of his Shide records, for insurance against earthquake effects has now become a large business. People interested in rebuilding cities, like San Francisco, asked his advice about new structures. Help of these and many other kinds was freely given, even when it involved the expenditure of much time and labour.

A list of his publications is given in an article by his lifelong friend, Dr. Woodward, in the 'Geological Magazine' of August, 1912. From 1874 to 1878, there are papers about Newfoundland, Cairo, the Sinaitic Peninsula, ice, icebergs, and volcanoes. In 1879, there are accounts of his journey to China and his cruise among the Kurile Islands; papers on volcanoes and on the Stone Age in Japan. In that year he published a small treatise on Crystallography. In 1880, volcanoes, the cooling of the earth, experiments on the elasticity of crystals, a list of Japanese minerals, stone implements in Japan, and three papers on earthquakes. From this time onwards his papers are almost all about earthquakes, and the list finished with one of very great labour, 'A Catalogue of Destructive Earthquakes, A.D. 7 to A.D. 1899,' published by the British Association in 1912.

He travelled in America and Canada; he travelled to Kamchatka, calling at most of the Kuriles (see 'Geol. Mag.,' 1879). He ventured into Korea from Siberia in 1878, when such a journey was very dangerous. He made observations in Manila, Borneo, the Australian Colonies, New Zealand, and many other islands of the Pacific. A very amusing anonymous book about Australia and New Zealand, which was frequently seen on railway book-stalls

about twenty-five years ago, at once revealed the name of its author to anybody who was well acquainted with Milne; it was full of humour and fun, but the most noticeable thing was that when the author came to any town, his first visit was to whatever there might be in the way of a museum.

Milne's success was greatly due to his power to interest all sorts of people in his work. Mrs. Hoover describes its effect on a travelling visitor. But it was something much deeper which gave to him the help of scientific men. He took much interest in all scientific work, and perhaps he thought too highly of the work of other men. He was very modest as to the value of his own services to the world. He grudged no time or trouble spent in helping other people when his help, scientific, social, or pecuniary, could be of value. Both in Japan and at Shide he was very hospitable. One who lived with him in great intimacy in Japan for nearly four years puts it on record that Milne never talked scandal nor detraction, and hated to listen to such things, and he cannot remember one expressed thought or action of Milne which was ungenerous or mean. Many people gave him admiration, but his intimate friends gave him affection also.

J. P.

JAMES GORDON MACGREGOR, 1852-1913.

JAMES GORDON MACGREGOR, successor of Tait as Professor of Natural Philosophy in the University of Edinburgh, was born on March 31, 1852, at Halifax, Nova Scotia, where his father was a minister of the Presbyterian Church. His early education was obtained in his native town, and in 1871, after graduating as Master of Arts at Dalhousie College, with the highest distinction, he was elected to a scholarship which required him to pursue scientific study in Europe. With this he proceeded to the University of Edinburgh. There the present writer first met him, as a fellow student in Tait's laboratory, and at Tait's suggestion they undertook a joint research "On the Electrical Conductivity of Certain Saline Solutions," the results of which were published in 1873 in the 'Transactions of the Royal Society of Edinburgh.' The experiments dealt with solutions of zinc and copper sulphate of various strengths, and a novelty in the method was the application of the Wheatstone balance so quickly after the current was established as to escape effects of "polarisation." The measurements showed that in zinc sulphate a maximum of electrical conductivity is passed at a strength considerably short of saturation. This research, the first on which MacGregor was engaged, determined the bent of much of his later work. From Edinburgh he went on to Leipzig and continued, under Gustav Wiedemann, the study of electrolytic resistance, a subject to which he frequently returned as affording matter for further experimental enquiry or discussion. The list of his published original papers comprises about 20 items dealing with this topic or with other closely related points arising out of his earliest research.

He became a Doctor of Science of London University in 1876 and returned to Halifax to take up a position in Dalhousie College as Lecturer of Physics. After holding this for a year he resigned it for a Science Mastership at Clifton College, Bristol, and two years later, in 1879, he again returned to Dalhousie, this time as Professor of Physics in a newly established Chair which his *alma mater* was glad to offer to her distinguished son. He remained Professor there for 22 years. During that time he took an active part in forming the Royal Society of Canada, and was a frequent contributor to its 'Transactions.' The summer vacations, especially in the earlier years of his Halifax professorship, he often spent in Edinburgh, working in Tait's laboratory and keeping up a close connection with Tait himself and with the band of young physicists whom Tait inspired. In 1901 the Edinburgh chair became vacant through Tait's resignation, and, difficult as it inevitably was to find a fitting successor, those who knew MacGregor felt there was a special appropriateness in the choice which gave him control of the laboratory where he had so long been a devoted worker. MacGregor's tenure of the professorship continued for 12 years, until it was terminated by his sudden death on May 21, 1913, at the age of 61.

Prior to his appointment, the physical laboratory at Edinburgh had been little more than a museum of lecture-room apparatus, with attics giving somewhat cramped and meagre opportunity to a few students to engage in research. To triumph over material obstacles such as the lack of appliances added zest and perhaps provided a stimulus that was not without value, and there was encouragement in the reflection that it was here that the work of Leslie, Forbes, and Tait had been done. But there was no laboratory of the modern type such as forms an instrument in the education of the ordinary student. With characteristic enthusiasm MacGregor set himself to supply the want. The removal of the Department of Physics from the old University Buildings to much larger quarters, in what had been the Edinburgh Infirmary, gave him the means of making a complete transformation and creating a well equipped laboratory, which, though his plans were somewhat curtailed for lack of funds, bears testimony to the completeness of his conception and his pervading care and prevision. Later, he took up the task of collecting from old students and others money with which to establish a Professorship in Mathematical Physics as a memorial to Tait.

In 1888 he married an Edinburgh lady, Miss Marion Taylor, who survives him with one son and one daughter.

His original contributions to science deal mainly with electrical conduction, the volumes of solutions, ionisation and its effect in depressing the freezing point. In other papers he discussed the foundations of dynamics, and his studies in this matter also found expression in a text-book of 'Kinematics and Dynamics,' first published in 1887. The treatment adopted there, while following the general lines of Thomson and Tait's 'Natural Philosophy,' is in many respects original, and is distinguished from that of most text-books by the severity of its logic and its independence of assumptions. From time to time he produced addresses and articles on various questions relating to education: these also are obvious products of a mind that was clearly no slave to convention, with an outlook as wide as it was original.

Whatever MacGregor undertook he pursued with untiring industry and with a thoroughness that left no loophole for intellectual dishonesty. He was unsparing of himself in his response to anything that he accepted as a call of duty. His attention to detail was perhaps sometimes so meticulous as to obscure larger issues, or at any rate to exhaust his strength. Never robust, he was compelled at more than one stage to treat himself for long as a semi-invalid, but he faced the handicap of indifferent health with the cheerful resolution that characterised his whole attitude to life, and he overcame it sufficiently to accomplish much. An admirable teacher, he inspired his students with something of his own zeal for knowledge, his own incapacity to accept half truths. All that was best in him was put at their disposal without reserve. He had no need to make himself accessible, for he was that by nature; his cordial manner only reflected a real geniality of soul.

MacGregor was a man with many friends who will long cherish the memory of a most lovable personality. The sunny brightness of his early

student days wonderfully survived into later life. It was a captivating quality, and with it there was frankness, unselfishness, insight, imperturbable good temper. He repaid friendship richly in the warmth of his own affection, in the readiness of his sympathy, in his constant willingness to help. His buoyancy stood him in good stead when, on the threshold of his career, he was warned that he had an ailment of the heart which made all effort dangerous. Though the sword did not fall then, nor till after many years, its presence must in some measure have checked his ambitions and limited his achievements. But he lived his life with a high courage, and if from time to time there was overstrain, it was of the body only: his spirit kept its elasticity to the end.

J. A. E.
